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THE CHEMISTRY OF COOKERY.

By W. MATTIEU WILLIAMS.

XXVII.

THE cookery of milk is very simple, but by no means unimportant. That there is an appreciable difference between raw and boiled milk may be proved by taking equal quantities of each (the boiled sample having been allowed to cool down), adding them to equal quantities of the same infusion of coffee, then critically tasting the mixtures. The difference is sufficient to have long since established the practice among all skilful cooks of scrupulously using boiled milk for making *café au lait*. I have tried a similar experiment on tea, and find that in this case the cold milk is preferable. Why this should be, why boiled milk should be better for coffee and raw milk for tea, I cannot tell. If any of my readers have not done so already, let them try similar experiments with condensed milk, and I have no doubt that the verdict of the majority will be that it is passable with coffee, but very objectionable in tea. This is milk that has been very much cooked.

The chief definable alteration effected by the boiling of milk is the coagulation of the small quantity of albumen which it contains. This rises as it becomes solidified, and forms a skin-like scum on the surface, which may be lifted with a spoon and eaten, as it is perfectly wholesome, and very nutritious.

If all the milk that is poured into London every morning were to flow down a single channel, it would form a respectable little rivulet. An interesting example of the self-adjusting operation of demand and supply is presented by the fact that, without any special legislation or any dictating official, the quantity required should thus flow with so little excess that, in spite of its perishable qualities, little or none is spoiled by souring, and yet at any moment anybody may buy a pennyworth within two or three hundred yards of any part of the great metropolis. There is no record of any single day on which the supply has failed, or even been sensibly deficient.

This is effected by drawing the supplies from a great number of independent sources, which are not likely to be simultaneously disturbed in the same direction. Coupled with this advantage is a serious danger. It has been unmistakably demonstrated that certain microbia (minute living abominations) which disseminate malignant diseases may live in milk, feed upon it, increase and multiply therein, and by it be transmitted to human beings with very serious and even fatal results.

I speak the more feelingly on this subject having very recently had painful experience of it. One of my sons went for a holiday to a farm-house in Shropshire, where many happy and health-giving holidays have been spent by all the members of my family. At the end of two or three weeks he was attacked by scarlet-fever, and suffered severely. He afterwards learned that the cow-boy had been ill, and further inquiry proved that his illness was scarlet fever, though not acknowledged to be such; that he had milked before the scaling of the skin that follows the eruption could have been completed, and it was therefore most probable that some of the scales from his hands fell into the milk. My son drank freely of uncooked milk, the other inmates of the farm drinking home-brewed beer, and only taking milk in tea or coffee hot enough to destroy the vitality of fever germs. He alone suffered. This infection was the more remarkable, inasmuch as a few months previously he had been assisting a medical man in a crowded part of London where scarlet fever was prevalent, and had come in frequent contact with patients in different stages of the disease.

Had the milk from this farm been sent to London in the usual manner in cans, and the contents of these particular cans mixed with those of the rest received by the vendor, the whole of his stock would have been infected. As some thousands of farms contribute to the supplying of London with milk, the risk of such contact with infected hands occurring occasionally in one or another of them is very great, and fully justifies me in urgently recommending the manager of every household to strictly enforce the boiling of every drop of milk that enters the house. At the temperature of 212° the vitality of all dangerous germs is destroyed, and the boiling point of milk is a little above 212°. The temperature of tea or coffee, as ordinarily used, may do it, but is not to be relied upon. I need only refer generally to the cases of wholesale infection that have recently been traced to the milk of particular dairies, as the particulars are familiar to all who read the newspapers.

It is an open question whether butter may or may not act as a dangerous carrier of such germs; whether they rise with the cream, survive the churning, and flourish among the fat. The subject is of vital importance, and yet, in spite of the research-fund of the Royal Society, the British Association, &c., we have no data upon which to base even an approximately sound conclusion.

We may theorise, of course; we may suppose that the bacteria, bacilli, &c., which we see under the microscope to be continually wriggling about or driving along are doing so in order to obtain fresh food from the surrounding liquid, and therefore that if imprisoned in butter they would languish and die. We may point to the analogies of ferment-germs which demand nitrogenous matter, and therefore suppose that the pestiferous wanderers cannot live upon a mere hydro-carbon like butter. On the other hand, we know that the germs of such things can remain dormant under conditions that are fatal to their parents, and develop forthwith when released and brought into new surroundings. These speculations are interesting enough, but in such a matter of life and death to ourselves and our children we require positive facts, direct microscopic evidence.

In the meantime the doubt is highly favourable to *bosch*. To illustrate this, let us suppose the case of a cow grazing

on a sewage-farm manured from a district on which enteric fever has existed. The cow lies down and its teats are soiled with liquid containing the germs which are so fearfully malignant when taken internally. In the course of milking a thousandth part of a grain of the infected matter containing a few hundred germs enters the milk, and these germs increase and multiply. The cream that rises carries some of them with it, and they are thus in the butter, either dead or alive, we know not which, but have to accept the risk.

Now, take the case of bosch. The cow is slaughtered. The waste fat, that before the days of palm oil and vaseline was sold for lubricating machinery, is skilfully prepared, made up into 2 lb. rolls, delicately wrapped in special muslin or prettily moulded and fitted into "Normandy" baskets. What is the risk in eating this?

None at all provided always the bosch is not adulterated with cream-butter. The special disease germs do not survive the chemistry of digestion, do not pass through the glandular tissues of the follicles that secrete the living fat, and therefore, even though the cow should have fed on sewage grass, moistened with infected sewage water, its fat would not be poisoned.

What we require in connection with this is commercial honesty, that the thousands of tons of bosch now annually made be sold as bosch, or if preferred as "oleomargarine," or "butterine," or any other name that shall tell the truth. In order to render such commercial honesty possible to shopkeepers, more intelligence is demanded among their customers. A dealer, on whom I can rely, told me lately that if he offered the bosch or butterine to his other customers as he was then offering it to me at 8½d. per lb. in 24 lb.-box, or 9d. retail, he could not possibly sell it, and his reputation would be injured by admitting that he kept it; but that the same people who would be disgusted with it at 9d. will buy it freely at double the price as prime Devonshire fresh butter; and, he added, significantly, "I cannot afford to lose my business and be ruined because my customers are fools." To pastrycooks and others in business, it is sold honestly enough for what it is, and used instead of butter.

Before leaving the subject of animal food I may say a few words on the latest, and perhaps the greatest, triumph of science in reference to food supply—*i.e.*, the successful solution of the great problem of preserving fresh meat for an almost indefinite length of time. It has long been known that meat which is frozen remains fresh. The Aberdeen whalers were in the habit of feasting their friends on returning home on joints that were taken out fresh from Aberdeen and kept frozen during a long Arctic voyage. In Norway game is shot at the end of autumn, and kept in a frozen state for consumption during the whole winter and far into the spring.

The early attempts to apply the freezing process for the carriage of fresh meat from South America and Australia by using ice, or freezing mixtures of ice and salt, failed, but now all the difficulties are overcome by a simple application of the great principle of the conservation of energy, whereby the burning of coal may be made to produce a degree of cold proportionate to the amount of heat it gives out in burning.

Carcasses of sheep are thereby frozen to stony hardness immediately they are slaughtered in New Zealand and Australia, and then packed in close refrigerated cars, carried to the ship, and there stowed in chambers refrigerated by the same means, and thus brought to England in the same state of stony hardness as that originally produced. I dined to-day on one of the legs of a sheep that I bought a week ago, and which was grazing at the Antipodes three months before. I prefer it to any English mutton ordinarily obtainable.

The grounds of this preference will be understood when I explain that English farmers who manufacture mutton as a primary product, kill their sheep as soon as they are full grown, when a year old or less. They cannot afford to feed a sheep for two years longer merely to improve its flavour without adding to its weight. Country gentlemen who do not care for expense, occasionally regale their friends on a haunch or saddle of three-year-old mutton, as a rare and costly luxury.

The antipodean graziers are wool growers. Until lately, mutton was merely used as manure, and even now it is but a secondary product. The wool crop improves year by year until the sheep is three or four years old; therefore, it is not slaughtered until this age is attained, and thus the sheep sent to England are similar to those of the country squire, and such as the English farmer could not send to market under eighteenpence per pound.

There is, however, one drawback; but I have tested it thoroughly, having supplied my own table during the last six months with no other mutton than that from New Zealand, and find it so trifling as to be imperceptible unless critically looked for. It is simply that in thawing, a small quantity of the juice of the meat oozes out. This is more than compensated by the superior richness and fulness of flavour of the meat itself, which is much darker in colour than young mutton.

GAMBLING SUPERSTITIONS.

By Richard A. Proctor.

(*Continued from page* 20.)

IT may appear paradoxical to say that chance results right themselves—nay, that there is an absolute certainty that in the long run they will occur as often (in proportion) as their respective chances warrant, and at the same time to assert that it is utterly useless for any gambler to trust to this circumstance. Yet not only is each statement true, but it is of first-rate importance in the study of our subject that the truth of each should be clearly recognised.

That the first statement is true, will perhaps not be questioned. The reasoning on which it is based would only suit the Mathematical Column; but it has been experimentally verified over and over again. Thus, if a coin be tossed many thousands of times, and the numbers of resulting "heads" and "tails" be noted, it is found, not necessarily that these numbers differ from each other by a very small quantity, but that their difference is small compared with either. In mathematical phrase, the two numbers are nearly in a ratio of equality. Again, if a die be tossed, say six million times, then, although there will not probably have been exactly a million throws of each face, yet the number of throws of each face will differ from a million by a quantity very small indeed compared with the total number of throws. So certain is this law, that it has been made the means of determining the real chances of an event, or of ascertaining facts which had been before unknown. Thus, De Morgan relates the following story in illustration of this law. He received it "from a distinguished naval officer, who was once employed to bring home a cargo of dollars." "At the end of the voyage," he says, "it was discovered that one of the boxes which contained them had been forced; and on making further search a large bag of

dollars was discovered in the possession of some one on board. The coins in the different boxes were a mixture of all manner of dates and sovereigns; and it occurred to the commander, that if the contents of the boxes were sorted, a comparison of the proportions of the different sorts in the bag, with those in the box which had been opened, would afford strong presumptive evidence one way or the other. This comparison was accordingly made, and the agreement between the distribution of the several coins in the bag and those in the box was such as to leave no doubt as to the former having formed a part of the latter." If the bag of stolen dollars had been a small one the inference would have been unsafe, but the great number of the dollars corresponded to a great number of chance trials; and as in such a large series of trials the several results would be sure to occur in numbers corresponding to their individual chances, it followed that the number of coins of the different kinds in the stolen lot would be proportional, or very nearly so, to the number of those respective coins in the forced box. Thus, in this case the thief increased the strength of the evidence against him by every dollar he added to his ill-gotten store.

We may mention, in passing, an even more curious application of this law, to no less a question than that much-talked-of but little understood problem, the squaring of the circle. It can be shown by mathematical reasoning, that, if a straight rod be so tossed at random into the air as to fall on a grating of equidistant parallel bars, the chance of the rod falling through depends on the length and thickness of the rod, the distance between the parallel bars, *and* the proportion in which the circumference of a circle exceeds the diameter. So that when the rod and grating have been carefully measured, it is only necessary to know the proportion just mentioned in order to calculate the chance of the rod falling through. But also, if we can learn in some other way the chance of the rod falling through, we can infer the proportion referred to. Now the law we are considering teaches us that if we only toss the rod often enough, the chance of its falling through will be indicated by the number of times it actually does fall through, compared with the total number of trials. Hence we can estimate the proportion in which the circumference of a circle exceeds the diameter by merely tossing a rod over a grating several thousand times, and counting how often it falls through. The experiment has been tried, and Professor De Morgan tells us that a very excellent evaluation of the celebrated proportion (the determination of which is equivalent in reality to squaring the circle) was the result.

And let it be noticed, in passing, that this inexorable law—for in its effect it is the most inflexible of all the laws of probability—shows how fatal it must be to contend long at any game of pure chance, where the odds are in favour of our opponent. For instance, let us assume for a moment that the assertion of the foreign gaming bankers is true, and that the chances are but from $1\frac{1}{4}$ to $2\frac{1}{2}$ per cent. in their favour. Yet in the long run, this percentage must manifest its effects. Where a few hundreds have been wagered the bank may not win $1\frac{1}{4}$ or $2\frac{1}{2}$ on each, or may lose considerably; but where thousands of hundreds are wagered, the bank will certainly win about their percentage, and the players will therefore lose to a corresponding extent. This is inevitable, so only that the play continue long enough. Now, it is sometimes forgotten that to ensure such gain to the bank, it is by no means necessary that the players should come prepared to stake so many hundreds of pounds. Those who sit down to play may not have a tithe of the sum necessary—if only wagered once—to ensure the success of the bank. But every florin the players bring with them may be, and commonly is, wagered over and over again. There is repeated gain and loss, and loss and gain; insomuch that the player who finally loses a hundred pounds, may have wagered in the course of the sitting a thousand or even many thousand pounds. Those fortunate beings who "break the bank" from time to time, may even have accomplished the feat of wagering millions during the process which ends in the final loss of the few thousands they may have begun with.

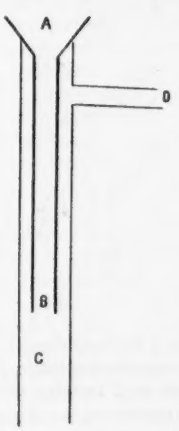
(To be continued).

PLEASANT HOURS WITH THE MICROSCOPE.

By HENRY J. SLACK, F.G.S., F.R.M.S.

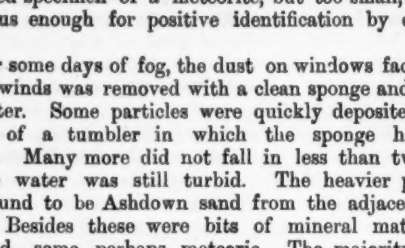
MUCH interest has been excited lately in the character of atmospheric dust, in the belief that we have been passing through a meteor stream of very minute bodies, which may have caused the splendid after-glows following sunsets, and the similar appearances before sunrise. Besides the investigations of Captain Noble and Mr. Mattieu Williams in this country, similar examinations of atmospheric dust on the Continent have led to like results; and it was stated in the French Academy that there was reason to believe an unusual quantity of fine meteoric matter fell in the second half of November and the beginning of December. Collecting snow as soon as it falls in the open country, letting it thaw in a perfectly clean vessel, and viewing the matter deposited under the microscope, seems the most successful way of catching these particles, free from much admixture; but at any time, in ordinary localities, the air is found to contain a multitude of small objects. In and near towns the quantity is much greater than away from them in open country places, or at considerable heights, far from habitations, where the air is often pure. Snow, however, in such places, would be likely to yield meteoric dust.

There are many ways of obtaining atmospheric dust. It may be swept from roofs, ledges in church-steeples, gutters, &c. Placing a sheet of glass, moistened with glycerine, so as to catch the wind, or putting the glass on the ground so as to receive whatever may fall perpendicularly, answers very well. Another plan is to use an aspirator, the principle of which is shown in the annexed diagram. It is evident that if a stream of water runs from A to B, and falls through C, it will carry with it air from C, which will be replaced by a fresh supply entering at D. If the tube D is stopped loosely with a tuft of pyroxilin, which is similar to gun-cotton, but less explosive, it filters the air and stops all solid particles. This substance readily dissolves in a mixture of strong alcohol and ether, and all particles not dissolved by those agents may be obtained by giving them time to deposit, or by filtering.

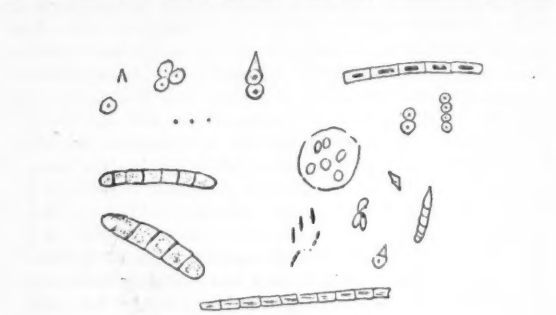


The writer was from home at the period of the snowfall which yielded meteoric matter to Captain Noble and Mr. Williams; but some water in a rain-gauge, the product of snow melting, did show some minute bodies like a finely-powdered specimen of a meteorite, but too small, and not numerous enough for positive identification by chemical means.

After some days of fog, the dust on windows facing prevailing winds was removed with a clean sponge and a little rain-water. Some particles were quickly deposited at the bottom of a tumbler in which the sponge had been drained. Many more did not fall in less than two days, and the water was still turbid. The heavier particles were found to be Ashdown sand from the adjacent open forest. Besides these were bits of mineral matter not identified—some, perhaps, meteoric. The majority of the objects were of vegetable nature, fresh-water algæ, spores of fungi, and an immense quantity too small for positive identification, but certainly organic. There were very few of the fragments of clothing common in the air of towns, few smuts, and few starch grains, which are sometimes plentiful



in less open places. The principal things discoverable with powers of half-inch, one-sixth, and one-eighth, magnifying respectively about 100, 320, and 450 linear, are shown in the figures. The objects in group A are vegetable × 100; B mineral, except the three-cornered ones, which are plant



hairs; C also mineral. In this damp season spores of fungi are deposited abundantly in all directions. If some of the green stuff forming thin layers on moist stones, or the green on fruit-trees, is scraped off, fungi spores will be found, as well as protococcus and other simple algæ. The minute algæ has a glaucous or a bright green tint, while most of the fungi spores are brownish.

If the remarkable glow aspects continue, or occur again, the search for meteoric dust will be of importance. M. Estrelle, writing in *Ciel et Terre*, observes that the zone of coloured light which all have admired is high up, "twenty kilomètres and more above the earth"—thirteen miles and more. The matter which causes it "is not water, because water at that height would be congealed by the cold that prevails in such regions. It is not ice, because ice-particles would make halos, and instead thereof a uniform tint has been seen without any symmetrical arrangement."

It is convenient to use a magnet in examining dust. If it attracts particles, they may or not be meteoric, and, on the other hand, they may be meteoric when not attracted. For example, I apply a magnet to a little powder from a large Australian meteorite, and no particle is attracted. Sometimes iron particles are carried a long way from smelting-works and engineering factories. Dust containing flattened iron particles, arising from the friction of locomotion on rails, has been found in railway carriages, together with others of a different shape, supposed to come from other sources.

The more free the air is from solid particles, the better for health. Many things found in air in and near towns, and especially the great manufacturing cities, are very mischievous as mechanical or chemical irritants; and such things as vegetable spores, even when not of a poisonous character, must interfere with the action of the delicate lung-cells. The enormous quantity of spores of various sorts that may be inhaled was shown by Dr. Angus Smith, who calculated from data obtained by examination of Manchester air, that, besides rubbish of all kinds, a man, in the course of ten hours, might breathe into his lungs 37½ millions of spores visible with a magnification of 1,600 diameters. The vitality of the lung-cells must be injured by such treatment, and rendered susceptible of disease, if any of the tubercle bacteria happen to enter them. Preparations of portions of human lung from workers in coal-mines look like miniature coal-cellars, and one wonders how life could be prolonged to even the low average that is attained under such conditions.

Starch cells in atmospheric dust are easily coloured blue with iodine. Sulphuric acid dissolves them, but does not readily destroy fungoid spores. Testing for iron is easy. A drop of dilute nitric acid will dissolve some of the metal, and ferrocyanate of potash readily gives the characteristic Prussian blue. Detecting a minute proportion of nickel in a very small quantity of meteoric material requires the practised skill of an experienced analyst. In summer time pollen grains are commonly found in the air. Hay fever is supposed to be caused by the pollen of grasses. Helmholz found a solution of quinine injected into the nose acted as a specific, but whether the pollen or some more minute organism causes the disease is still doubtful.

WILD BEES.

BY S. A. BUTLER, B.A., B.SC.

TO most people, in all probability, the expression "*wild* bees" is nothing more than a name. We are all familiar enough with the honey-collecting and comb-forming propensities of the industrious communities which have for so many ages been domesticated, so to speak, by man; and as these particular habits of this particular species form its sole commercial recommendation, the word "bee" usually suggests, in this utilitarian age, only ideas of that "little

busy bee," which, according to the poet, if not according to the naturalist, delights to—

"Gather honey all the day
From every opening flower."

At the outset, therefore, let me say that I shall make no reference to the species, *Apis mellifica*, the Honey, or Hive Bee, which is the inhabitant of the hives of our thrifty villagers. These are not really *wild* bees; they can scarcely merit the epithet while their convenience and wellbeing are so carefully studied as in the modern apiarium. But there are to be found in the woods and fields of our country upwards of 200 different kinds of insects which structurally bear a close resemblance to our friends of the hive, but lead an existence entirely independent of the cherishing care of man, and are therefore truly "wild bees." Many of them are somewhat obscure-looking insects, and, when on the wing, might readily be mistaken for hive bees, or even for flies. The minute details of structure, which are often of the greatest significance with respect to the habits of the insects, and which, on an attentive and close examination, sufficiently separate them from the hive bee, cannot, of course, be discerned by the hasty glance which is all that is possible as the insect buzzes past on its industrious flight. To gain an intimate knowledge of these insects, as of all others, we must catch them, minutely examine them, even with the aid of a microscope, pull them to pieces, compare them with one another part by part; and when we have done this, we shall be struck with the extraordinary diversity that exists under the external aspect of similarity. It will be well to notice some structural points that all bees have in common, that we may form a clear conception of what a bee is really like, and may be able to recognise such insects when we meet with them in our country rambles—not so easy a task, perhaps, as it may seem at first sight.

In the first place, a bee is a hymenopterous insect, *i.e.*, it has four membranous wings, the fore pair being much larger than the others. Now, this alone will serve to distinguish bees from certain two-winged flies that are very generally mistaken for bees. It should be mentioned, however, that when the wings are expanded, the hind pair are so closely hooked on to the fore pair that the two may easily be mistaken for a single wing. But it will not do to rely wholly upon the number, relative size, and texture of the wings; there are hundreds of other insects that have wings of the same kind; there is one point about these organs, however, that should be attended to, as it will often help to show what insects are *not* bees, even if it does not determine what are: I refer to the neuration of the wings. The rays which form the strength and support of the wings are very constant in their arrangement in the same species, and generally in the same genus, while differing considerably in different genera. They are not, as many might suppose, set in the wing at random; but there is always some definite arrangement of them—so definite, indeed, as to render possible the giving of names both to the nervures and to the cells they form by their intercrossing. Towards the tip of the fore-wing (Fig. 1), on the front margin, there are two such cells, the one nearer the body opaque, and called the stigma; and the other transparent, and called the marginal cell. Below these there are always two or three other cells placed one after another in a row; these are called submarginal cells, and no bee has less than two or more than three of these; if, therefore, you find an insect that, in other respects, looks something like a bee, and yet has only one submarginal cell, you will know that this is the key to other important differences which a careful inspection will reveal, and which remove the creature—not, indeed, from the Hymenoptera, but from that section of the order which contains the bees. All the most familiar bees, such as the hive bee, the bumble-bee, the Andrenæ, Halicti, and Anthophoræ, have three submarginal cells; it does not follow, however, that if a hymenopterous insect has two or three submarginal cells, it is therefore a bee.

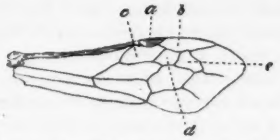


Fig. 1.—Fore-wing of Andrena pilipes.
a, stigma. *b*, marginal cell.
c, *d*, *e*, submarginal cells.

Fig. 2.—Tongue of Colletes Daviesana, a Plastering Bee.

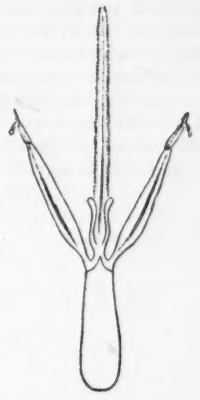


Fig. 3.—Tongue of Bombus terrestris, a yellow-tailed Humble-Bee.

Bees have a pair of powerful mandibles, accompanied by a licking apparatus of somewhat complicated structure, and often of considerable length, and called popularly the tongue, technically the labium (Figs 2 and 3). This structure, especially in its longer forms, is very characteristic of the group. The antennæ are stout, and generally of moderate length, but longer in the males than in the females. The sting is a curiously-modified ovipositor, and therefore exists only in the females. The males, which may generally be recognised by the superior length of their antennæ, may be handled with impunity, for they are quite unable to inflict any injury. All bees are vegetarians, their food consisting principally of pollen. The three so-called sexes—viz, males, females, and neuters—are to be found only amongst those few bees that are social—*i.e.*, form large communities living in the same nest. You will look in vain in the nests of other bees for neuters; only males and females are to be found. The greater number of bees have some part or other of the body clothed with hairs, longer or shorter, called collectively "pubescence." One very important use of this is to assist in the collection or conveyance of pollen. Such are the principal characteristics of bees, and attention to them can scarcely fail to lead the observer to the correct separation of bees from the rest of the Hymenoptera.

But the creatures are not always thus. During the course of their life they pass through a series of marvellous changes. There is first the egg, then the grub or larva, then a motionless little doll or pupa, and then, finally, the fully-developed bee. Before the little creature commences its existence as an egg, however, many preparations have to be made by its mother for its reception into the world and its nourishment therein; in fact, with the deposition of the egg the maternal duties cease. First, a suitable situation has to be selected for the home of the expected offspring. The nature of the spot fixed upon will

vary with the species; some choose a sandy bank, others a dead bramble stick, others again the chinks in a wall, and yet others an old snail-shell; in fact, there are very few things that offer cavities of any kind which may not be selected by some species or other. If there is no cavity ready-formed, in which to place the egg, the mother must set to work to make one; this she does by means of her powerful mandibles, and a substance must be hard indeed to resist the incessant attacks of these powerful weapons. By dint of continuous snippings, she bites out fragment after fragment of the rock or wood, until a hole large enough to admit her body is made. Using this as a starting-point, she tunnels out to the depth of several inches—sometimes a single burrow, sometimes a number of ramifying channels—carefully smoothing off all asperities as she proceeds. The burrows are frequently lined, either with a kind of silk secreted by the insect, or with fragments of leaves, all cut most carefully to pattern, and laid so as to overlap.

(*To be continued.*)

HOW TO CHOOSE A TRICYCLE.

By John Browning,

Chairman of the London Tricycle Club.

TAKE care that any machine you purchase is provided with good foot-rests, for the purpose of putting the feet on when running down hill. These should be well raised, so as to bring the feet as high as possible, as a change from the vertical position they should take when pedalling, and they should be covered with stout indiarubber tube, to prevent jarring the feet as much as possible. In this respect they will be improved if, instead of being attached directly to the front portion of the frame which carries the fork of the steering-wheel, they are mounted on a strong flat spring. These remarks apply only to machines of the Salvo type. It is a good plan to have the pedals run on a clutch. This is a contrivance by which the pedals drive the lower chain wheel continually when you are pedalling forward; but on descending an incline, as soon as the machine is running as fast as you are pedalling, the pedals stand still, and become available as foot-rests. The clutch is also extremely useful in another way; it will save you from ever becoming fixed when riding up a hill or working over rough ground, by getting your machine on dead centres—that is one pedal at the top and the other at the bottom—in which position you would probably have to dismount from inability to move the machine. A slight turn backwards of one of the cranks when you are using a clutch brings it into a position of full power. Of course, it is impossible to back the machine when it has a clutch as just described unless it is provided with some contrivance for throwing the clutch out of action; but this is of little or no consequence unless the machine has to be used constantly in the traffic of crowded thoroughfares.

I reckon a good clutch worth about one mile in ten, probably; and certainly worth one mile in fifteen, at least, when riding over constantly undulating roads.

I wish it to be distinctly understood that throughout my articles I exclude both the "Humber" and the "Coventry" when I speak generally of front or rear-steering machines. Each of these machines may best be considered as constituting a class in itself, though I would respectfully submit to those who dissent from the views I have expressed as to the efficiency of machines with small wheels, that even the "Humber" tricycle is not exempt from the laws which govern Mechanics.

I find from a comparison of a number of notes that I have made, that for every decrease of 2 lb. in the weight of a tricycle the machine will travel roughly about one mile further in a day without extra exertion, so that if a rider can ride fifty miles in a day on a machine weighing 90 lb., he will be able to ride sixty miles on a machine weighing 70 lb.

It is a commonly accepted notion that little or no weight must be thrown on the steering-wheel. This is a complete mistake. A certain amount of weight may be thrown upon the steering-wheel with advantage. But to get the best results the steering-wheel should be larger and the driving-wheel should be smaller than they are now generally made. We should have machines with 20-in. steering-wheels and 40-in. driving-wheels, instead of 14-in. steering-wheels and 48-in. driving-wheels as at present. A reduction in the size of the driving-wheels of 2 in. saves pounds in weight, while a reduction in the steering-wheel of 2 in. only saves a few ounces.

Have the rubber tyre of the steering-wheel at least one-eighth of an inch larger than the tyres of the driving-wheels, as it will lessen vibration greatly. Manufacturers commonly make the rubber tyre on the steering-wheel smaller than those on the driving-wheels. Do not have such a machine.

In my "Cobweb" sociable I have had the steering-wheel made 18 in. and the driving-wheels 36 in. The tyre of the steering-wheel is seven-eighths, and those on the driving-wheels are three-quarters. The weight of the riders and machine is 332 lb., and the weight on the front wheel is 85 lb. This machine drives much more easily than any other sociable I have ridden.

Choose a machine which is enamelled plain black with Harrington's enamel. It has a very superior look to paint, it seldom gets rubbed off, and if it does get injured you can easily make good the injury yourself with a little of the best black varnish enamel, sold by all tricycle agents.

Do not have a lot of nickelised work about the machine. It is a deal of trouble to keep it clean, and it is scarcely any protection to the steel work against rusting.

If you should think of testing a tricycle before you buy one, which I think is very probable, mind you begin first by practising on a nearly level road. Do not attempt to ride down a steep hill, *nor yet up one.* Either is likely to prove dangerous until you can pedal strongly and evenly, steer well, and use the brake. I can imagine some reader asking, What danger can there be in trying to ride up a steep hill? Why, it is that if you come to a standstill from finding the work too severe for you, and the machine begins to run backwards, and you attempt to stop it by putting the brake on, you will find either that it will not stop the machine instantly, or that, if it does, the machine will turn over backwards upon you.

In my next article I will relate a few of the results of my experiments in testing tricycles, so that others may profit by my experience.

VARIATION IN ANIMALS.

A MULTITUDE of variations have arisen, for instance in colour and in the character of the hair, feathers, horns, &c., which are quite independent of habit and of use in previous generations. It seems far from wonderful, considering the complex conditions to which the whole organisation is exposed during the successive stages of its development from the germ, that every part should be liable to occasional modifications: the wonder indeed is that any

two individuals of the same species are at all closely alike. If this be admitted, why should not the brain, as well as all other parts of the body, sometimes vary in a slight degree, independently of useful experience and habit? Those physiologists, and there are many, who believe that a new mental characteristic cannot be transmitted to the child except through some modification of that material substratum which proceeds from the parents, and from which the brain of the child is ultimately developed, will not doubt that any cause which affects its development may, and often will, modify the transmitted mental characters. With species in a state of nature such modifications or variations would commonly lead to the partial or complete loss of an instinct, or to its perversion; and the individual would suffer. But if under the then existing conditions any such mental variation was serviceable, it would be preserved and fixed, and would ultimately become common to all the members of the species.—CHARLES DARWIN.

TUMBLER PIGEONS.

THE tumbling of the pigeon is a habit which, if seen in a wild bird, would certainly have been called instinctive; more especially if, as has been asserted, it aids these birds in escaping from hawks. There must have been some physical cause which induced the first tumbler to spend its activity in a manner unlike that of any other bird in the world. The behaviour of the ground-tumbler or Lotan of India, renders it highly probable that in this sub-breed the tumbling is due to some affection of the brain, which has been transmitted from before the year 1600 to the present day. It is only necessary gently to shake these birds, or, in the case of the Kalmi Lotan, to touch them on the neck with a wand, in order to make them begin rolling over backwards on the ground. This they continue to do with extraordinary rapidity, until they are utterly exhausted, or even, as some say, until they die, unless they are taken up, held in the hands, and soothed; and then they recover. It is well known that certain lesions of the brain, or internal parasites, cause animals to turn incessantly round and round, either to the right or left, sometimes accompanied by a backward movement: and Mr. W. J. Moore (*Indian Medical Gazette*, Jan. and Feb., 1873) gives an account of the somewhat analogous result which followed from pricking the base of the brain of a pigeon with a needle. Birds thus treated roll over backwards in convulsions, in exactly the same manner as do the ground-tumblers; and the same effect is produced by giving them hydrocyanic acid with strychnine. One pigeon which had its brain thus pricked recovered perfectly, but continued ever afterwards to perform summersaults like a tumbler, though not belonging to any tumbling breed. The movement appears to be of the nature of a recurrent spasm or convulsion, which throws the bird backwards, as in tetanus; it then recovers its balance, and is again thrown backwards. Whether this tendency originated from some accidental injury, or, as seems more probable, from some morbid affection of the brain, cannot be told; but at the present time the affection can hardly be called morbid in the case of common tumblers, as these birds are perfectly healthy and seem to enjoy performing their feats, or, as an old writer expresses it, "showing like footballs in the air." The habit apparently can be controlled to a certain extent by the will. But what more particularly concerns us is that it is strictly inherited. Young birds reared in an aviary which have never seen a pigeon tumble, take to it when first let free. The habit also varies much in degree in different individuals and in different sub-breeds; and it can be greatly augmented by continued selection, as seen in the house-tumblers, which can hardly rise more than a foot or two above the ground without going head over heels in the air. —CHARLES DARWIN.

THE CANADIAN PORCUPINE.

FROM time immemorial the belief has existed that the porcupine can project its quills through the air like arrows at an enemy, and beyond this the popular mind is yet more in error as regards the structure and habits of this aberrant and curious mammal. Let us therefore consider some of the more prominent points in its life history.

The female porcupine during the last of April or the first of May builds a rough nest in some hollow tree or rock fissure, and there brings forth usually two, sometimes three, young ones. The mother is exceedingly shy until the young are weaned, and but few observations have been made upon them during the period of suckling: probably like all rodents they mature very rapidly and are soon able to shift for themselves. This species is one of the slowest and most clumsy of quadrupeds; safe in its protective armour, it seldom makes much effort to escape when surprised on the ground, but placing its muzzle between its fore legs, erecting its spines, and whisking rapidly its short tail, waits on the defensive—and even the panther and formidable grizzly bear are obliged to retreat from this fine array of bayonets.

The spines vary much in size and shape, varying from the coarse brown hair with which they are mingled to strong three-inch spikes, one-eighth of an inch in diameter. Their bases are white, and the points dark brown, the latter portion well provided with sharp, recurved barbs. Being but loosely rooted in the skin, when roughly touched the points penetrate, the barb holds fast, and the quill comes off attached to the offending body; doubtless from this arose the fable that the animal can *shoot* its quills. When the sharp spines once penetrate the skin of an animal, owing to the peculiar set of the barbs, the muscular movements of the wounded part cause them to work their way inward, and a very serious wound is finally the result. Panthers, wolves, and wild-cats have frequently been found dead with hundreds of quills embedded in their fore feet and mouths, thus proving fatal. Dogs are also frequently killed and injured, and, in consequence, the porcupine is hated and always mercilessly killed by hunters whenever found. The food of the hedgehog—as the porcupine is almost universally called by woodsmen—consists of the inner bark, and at times the leaves, of trees. When pressed by hunger it will devour the bark of almost any species, but the hemlock and spruce seem to constitute its favourite food. The young and succulent trees are usually the ones selected, and the animal seldom leaves one until it has been entirely stripped of its bark. But the porcupine seems to be almost omnivorous, for in captivity it will eat almost any vegetable substance. In the Adirondack wilderness—where this species abounds—they are frequent visitors at deserted camps, trying their powerful incisor teeth on all that comes in their way. It is exceedingly unsafe to leave one of the light cedar canoes there used anywhere in the woods unguarded for a day or two, for the hedgehog seems to have a decided liking for oil paint and varnish, and will cut down the entire side of a boat in a very few days. I have seen many boats so rendered useless. As may be imagined, they are not much beloved by the guides, among whom "the d—d hedgehogs" is a favourite topic on which to let off steam when a boat leaks.

As already suggested, the porcupine is a capital tree-climber, its strong, hand-like fore feet and long claws being perfectly adapted for the purpose. It uses these paws to hold food when eating, sitting on its haunches in the manner of a squirrel.

It does not hibernate, but remains active during the winter, clearing the snow away from the tree branches, and living entirely on their bark. At times it forms a den in a hollow tree near its feeding grounds in which to pass the night.

The porcupine quill-work of the Indians—the quills being stained various colours—is too well known to need description. Its flesh is also eaten by both whites and Indians, and is said to resemble pork.

This species becomes very tame and gentle in confinement, readily learning to take food from the hand, and never elevating its quills when stroked or taken in the arms of those who are kind to it.

The Canada Porcupine.

The Canada porcupine (the *Erethizon dorsatus* of zoologists) scarce needs any description; a short, heavily-built animal, 38 in. in entire length, with a short tail, huge yellow incisor rodent teeth, two above and two below, the skin provided with a thick mat of erectile spines, are sufficiently prominent characters to identify the animal at a single glance.

The genus *Erethizon* presents many interesting details of structure. As is the case in the beaver, the molar teeth resemble in structure those of the horse, being formed of complex infoldings of dentine bounded with enamel, and the valleys between filled with cementum—the best arrangement imaginable for grinding thoroughly its coarse and fibrous food. It is exceedingly interesting that the beaver, feeding on the same substances, should present the same tooth structure.

The infraorbital foramen—in most mammals of very small size and transmitting only the infraorbital nerve and blood-vessels—is of enormous size, and through it passes the great muscle which closes the lower law—the masseter; by this arrangement great leverage and strength is given to the inferior jaw.

A porcupine found in Java shows the curious anomaly of a tongue provided with tough, horny plates, but this is not the case with the American representative of the genus. The right lung of the *Erethizon* is stated to be twice the size of the left, but my own dissections would not put the limit at more than one-third larger.

The whole muscular system is exceedingly well developed, and the skin is well supplied with powerful special muscles to erect the spines.

The Canada porcupine is essentially a northern animal, seldom being found as far south as Virginia. A western variety is said to be found as far south as Mexico, but only, I believe, on high plateaus of temperate climate. It has been found as far north as latitude 67°. In the North Woods of New York State, as already stated, I have found it abundant; a few yet remain in the wildest portions of Pennsylvania; but this is one of the many animals doomed to rapid extinction, and every year it becomes rarer.—*Scientific American.*

THE UNIVERSE OF SUNS.

By Richard A. Proctor.

(*Continued from p.* 35.)

In passing to the work of Sir W. Herschel as a student of the constitution of the universe, I cannot but express some degree of surprise that so little has been done to bring the records of his labours properly before the students of astronomy. His papers merely collected into a volume would form a most important accession to astronomical literature. But if suitably edited and illustrated by the work of his son and of others who have succeeded him in his own field of labour, the volumes would do more to advance the study of sidereal astronomy than any work which has been published during the last century. What has hitherto been done in making Herschel's words and work public (in England at least) has been rather an injustice than otherwise to his memory.* It seems to have been supposed that Herschel's own account might be treated as we should treat such a work as his son's "Outlines of Astronomy," that extracts might be made from any part of any paper without reference to the position which that paper chanced to occupy in the complete series. It does not seem to have been noticed that not only was there a progression in the ideas (as well as in the work) of the great astronomer, but that there was a complete change in his opinions during the long course of his labours. Hence views expressed by him in his earlier papers are not uncommonly in strong contrast with those which he advocated in later years; opinions which he regarded as almost certainly just at one time were rejected as most probably incorrect after a few years of fresh labour; and whereas in 1785 he enunciated with some definiteness a theory respecting the constitution of the universe, he not only abandoned this theory (implicitly) in 1811, not only gave up the very principles on which it had been based, but he did not consider himself in a position during any subsequent part of his career to

* I except of course the account given by Sir J. Herschel of his father's work, as well as the account in Grant's "History of Physical Astronomy." Neither of these accounts is however complete, though so far as they extend they are accurate.

state with the same degree of definiteness any theory whatever respecting the constitution of the heavens.

It is to be premised, however, that even the theory of 1785 is not properly described in most of our textbooks of astronomy, and that some of the descriptions are the merest travesties of the noble conceptions of Sir W. Herschel. This will appear from his own words, which I shall quote as freely as space will permit.

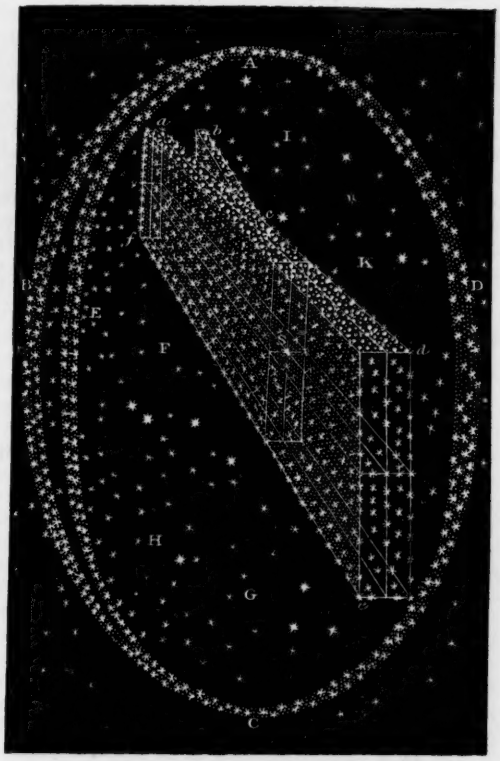


Fig. 4. Illustrating Sir W. Herschel's views in 1784.

a given considerable distance from one another, and calling this a sidereal stratum, an eye placed somewhere within it will see all the stars in the direction of the planes of the stratum projected into a great circle, which will appear lucid on account of the accumulation of the stars, while the rest of the heavens at the side will only seem to be scattered over with constellations, more or less crowded, according to the distance of the planes or number of stars contained in the thickness or sides of the stratum."

"Thus, if the solar system be supposed at S, in the middle of the nebula *a b c d e f*, with two branches, *a c*, *b c* (Fig. 4), the nebula will be projected into a circle A B C D, the arches A B C, A E C, being the projection of the branches *a c*, *b c*. while the stars near the sides of the stratum will be seen scattered over the remaining part of the heavens among the spaces F, I, H, K, G. If the eye were placed somewhere without the stratum, at no very great distance, the appearance of the stars within it would assume the form of one of the lesser circles of the sphere, which would be more or less contracted according to the distance of the eye; and if this distance were exceedingly increased, the whole stratum might at last be drawn together into a lucid spot of any shape, according to the position, length, and height of the stratum."

Such was the idea broached in the paper of 1784, which has been absurdly combined with the paper of 1785, as if the two presented the same views. There can be no question that Herschel himself regarded the earlier paper as merely preliminary. The observed results described in the paper of 1785, as well as most of the inferences, are by no means in strict agreement with the paper of the preceding year. But Herschel has not thought it necessary to dwell on this point, not supposing it would seem that the paper of 1784 would one day be amalgamated with that of 1785, as though the two were on the same footing.*

* Even Arago in his Analysis of Herschel's Life and Labours, has fallen into this strange mistake; and he has been followed by all the French writers of popular treatises on astronomy. At p. 456, he says that our galactic system is a hundred times more extended in one direction than in another; and then he refers to a figure of three dimensions, constructed by Herschel on the basis of his observations. Struve justly remarks that there is a misapprehension here, since the proportion of the greatest extension to the least, in the only section Herschel based on observation, is not 100 to 1, but barely 5½ to 1. "There is also another mistake, it appears to me," adds Struve, "in Arago's Analysis. Herschel only gives a section of the Milky Way in the Memoir of 1785; in that of 1784 there is indeed a figure of three dimensions; but this figure is only given to explain the theory, though it does in some sense correspond with the ideas Herschel had formed respecting the Milky Way." For comparison with the fanciful figure here referred to, I give here, a little before its time, a picture (Fig. 5) illustrating the *results* which Sir W. Herschel was able to announce in 1785.

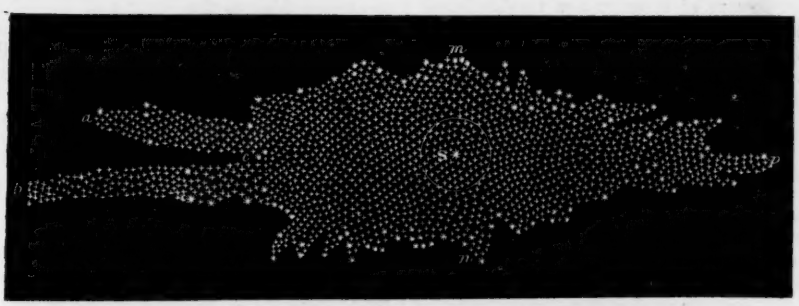


Fig. 5. Illustrating Sir W. Herschel's observations up to 1785.

In the year 1784 Sir W. Herschel advanced, but only in a preliminary way, the hypothesis that the Milky Way is to be regarded as the projection of our stellar system on the celestial sphere. "We gather this," he said, "from the appearance of the galaxy, which seems to encompass the whole heavens, as it certainly must do if the sun is within the same; for suppose a number of stars arranged between two parallel planes indefinitely extended every way, but at

This may be inferred from his remarks in the paper of 1785 respecting the relative position of observation and theory—remarks which are in other respects well worth quoting. "First let me mention," he says, "that if we would hope to make any progress in an investigation of this delicate nature, we ought to avoid two opposite extremes, of which it would be hard to say which is the more dangerous. If we indulge in fanciful imagination, and build worlds of our own, we must not wonder at our going wide from the path of truth and nature; but these will vanish like the Cartesian vortices, that soon gave way when better theories were offered. On the other hand, if we add observation on observation without attempting to draw not only certain conclusions, but also conjectural views from them, we offend against the very end for which only observations ought to be made. I will endeavour to keep a proper medium; but if I should deviate from that I could wish not to fall into the latter error."

(To be continued.)

CURIOUS PHENOMENON.—At 5.25 p.m. on Friday, the 11th inst., a ball of light, shaped like a pear, with the broad end downwards, was seen from Fort William as if suspended midway between Ben Nevis and the Caledonian Valley. It descended till near the surface of the earth, and then it burst, lighting the whole valley. In colour it resembled the electric light.—*The Electrician.*

PROFESSOR LOISETTE'S SYSTEM OF MEMORY-TRAINING.—Since the appearance of a paragraph in KNOWLEDGE mentioning (with my sanction) that an Assistant of "ours" had tested Professor Loisette's method of training the memory, with most satisfactory results, I have received several hundreds of letters asking for my own opinion of the system. To these I have been unable to reply, first because I did not know what Professor Loisette's method might be, and secondly because the letters reached me in too great abundance. But wishing to respond to these correspondents I have taken advantage of the period of rest from travel recently enjoined me, to examine thoroughly the Loisettian system. I have not been content to try it in my own case only, but knowing how greatly memories differ in quality, I have made the inquiry in company with two relatives, whose powers of memory are very different from mine—in some points superior, in others I think not quite so good. The result in every case has been to confirm fully what my friend Dr. Andrew Wilson has said respecting the system. Whether regarded as a device for memorising, or in its more important aspect as a system of memory-training, Prof. Loisette's method appears to me admirable. I have tested it in my own case on those matters in which my memory is least trustworthy, perhaps because least exercised; and I have been surprised to find how easily and pleasantly I can fix such matters in my mind, almost without an effort, yet in such a way that I am satisfied they are there for good. My two companions in the inquiry have had in one sense the same experience, in another sense an experience entirely different; they have tested the system on entirely different subjects, but with the same satisfactory results. I have no hesitation in thoroughly recommending the system to all who are in earnest in wishing to train their memories effectively, and are therefore willing to take reasonable pains to obtain so useful a result. —R. A. PROCTOR.

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Reviews.

RED DEER.*

A CHARMING work by the author of "The Gamekeeper at Home." It is full of interest, and as instructive as interesting. Mr. Jefferies "can well of vénerie." The chapter on the Ways of Deer is especially delightful. Here is a story which would be thought rather startling in a novel, but being true is naturally more surprising than an invented tale:—"It happened once that a 'forester' was discovered in a certain district" (a "forester," urban reader, is the old name for a stag), "and a party was quickly formed to go out and shoot the stag. Among those who went was a man well known as a successful deer shot, upon whose aim they chiefly relied. They took with them a gallon of spirits. After some time spent in searching for the stag, and just as they were beginning to weary of the attempt, up the 'forester' jumped close to the party. A volley was fired—the muzzles almost touching the stag—but the game went off at full speed. The old gunner declared that he had hit the mark, he was sure he had aimed straight. In a minute or two as they watched the stag bounding up the hill a mile away, suddenly he dropped and lay still, evidently dead. It was found that the ball from the old gunner's weapon had grazed the stag's heart, and yet with that wound he had run upwards of a mile. No other bullet had struck him"—though the shooters were so close around him that they narrowly escaped wounding each other!—"Now" follows the dim horror of the tale": the opening is rather absurd than otherwise—"The party were so tired of walking after the stag that they did not go at once to ascertain if he was really dead or to cut his throat. They sat down in the heather to refresh themselves with the spirits, and so well did they do this that by and by the old gunner fell firm" (*sic*) "asleep. Neither blows nor shouts could arouse him, so in *order to wake him up* they set fire to the heather. Dry as tinder, the heather blazed up in such a fury of flame that they fled aside to get out of the way, leaving the sleeper to his fate. The flame passed over him as he lay, and when the wind had driven it along they found him in his burning clothes. They could not put the burning clothes out, and so carried him to the river and dipped him in. He was terribly scorched and half drowned" (we presume he was by this time awake) "and was long ill, but ultimately recovered."

WORLD LIFE.†

IN this work Prof. Winchell combines together a number of known facts bearing directly or indirectly, or not at all, on the past development of the universe with a number of calculations based on various theories more or less (generally less) reconcilable with known physical laws but quite irreconcilable *inter se*, and to his satisfaction reconciles everything with his own ideas about the past of the planetary system. Hinrichs and Ennis, Alexander and Slaughter, Trowbridge, Twisden and a host of others who have put more or less grain-like chaff into fine grinding-mills and have gotten out what they take for pure flour, are questioned, quoted, and, where their results cannot be reconciled with his own, corrected by the unerring author. The nebular hypothesis which Laplace threw out only as a

* "Red Deer." By RICHARD JEFFERIES. (Longmans, Green, & Co., London.)

† "World Life; or Comparative Geology." By Prof. ALEX. WINCHELL, LL.D. (London: Trübner & Co.)

speculation, which has been disproved along its whole length by modern physical inquiries, is taken as the foundation for a superstructure which even such a theory as Newton's gravitation would not bear. The time has not nearly come yet—perhaps it will never come—when science will be able to speak so definitely and clearly about the past of the universe as Prof. Winchell here claims to do. Yet the matter here collected would be valuable if it had not been so wildly scattered as it is.

The work is full of errors of all sorts and sizes, from the initial error of taking sand for the foundation of what is meant to be a magnificent structure down to such trivial though significant mistakes as mis-spelling names (Mitchel for Michell, Rutherford for Rutherfurd, &c.) speaking of Neison's treatise "Der Mond" whose "Moon" is a well known English work, and in other ways showing how little original inquiry our author has made into the matters of which he speaks with such cool confidence.

We much prefer Professor Winchell's views on Geology to his fancies about matters astronomical.

NOTES ON BOOKS.

THE TRANSIT INSTRUMENT; a manual of the Transit Instrument, as used for obtaining correct time, by *Latimer Clark*, M.I.C.E. (Messrs. E. & F. N. Spon, London.) A useful and trustworthy little work. — WHERE SHALL I EDUCATE MY SON, a manual for parents of moderate means, by *Charles Eyre Pascoe* (Houlston & Sons, London). A very useful treatise, and so far as we have been able to test the statements, reliable: the study of the work will sadden those who know something of educational opportunities abroad, and especially across the Atlantic. — TRUE AND FALSE ISSUES BETWEEN CHRISTIANITY AND SCIENCE. By the *Rev. T. Blackburn*, B.A. (W. Skeffington & Son, London). A well meant and moderately toned work of a kind liked by many, but outside our scope.—THE EARTH AND THE SOLAR SYSTEM (Moffatt & Paige, London). We learn from this work that longitude is determined by observing the exact moment when the sun attains his greatest altitude; that the earth is kept in her track by two antagonistic forces one centripetal the other centrifugal; that the earth turns on her axis in twenty-four hours; that the Pole-star remains fixed in the sky; that the distance from the Sun to Mars (see p. 51) is about the same as from Mars to the orbit of Uranus; with several other remarkable matters which surprise us a little. The anonymous author of this little work would have done well to obtain the excellent American edition of Mr. Lockyer's "Elementary Lessons in Astronomy," instead of trusting as he appears to have done, to the original version.—GREEK AND ROMAN COINS, Handbook of, by *B. V. Head*, M.R.A.S. (W. Swan Sonnenschein & Co., London). An excellent little guide (illustrated) for young collectors. Price one penny only.—PHOTOGRAPHY, Amateur's first Handbook. By *J. H. T. Ellerbeck* (D. H. Cussons & Co., Liverpool). A very useful work containing many valuable hints suggested and confirmed by experience. Sufficiently illustrated. — DR. CORPUS'S CLASS (Wyman & Sons, London). A series of nine more or less laughable lectures on the human body from what the author regards as a comic point of view. The work merits fairly what Wendell Holmes calls the "laugh eleemosynary."—WIESEN as a Health-Resort in early Phthisis, by *Dr. A. T. Tucker*. (Baillière Tindall & Cox, London). With the increasing popularity of the high altitude system of treatment of early phthisis books of this class have become not only useful but essential. This work is obviously the result of careful study. The chapter on personal health-treatment is especially valuable.—CLEOPATRA'S NEEDLE, by the *Rev James King*, M.A. (The Religious Tract Society, London) contains an interesting account of Hieroglyphics, and gives the full interpretation (with careful drawing) of each face of Cleopatra's Needle. Londoners expecting visits of country cousins, should master these interpretations. The remarks on Egyptian religious views seem narrow, when we consider how much that is now valued was derived from the land of the Pharaohs—KASHGARIA (Eastern or Chinese Turkistan) translated from the Russian of *Colonel A. N. Kuropatkin*, by *Major Walter E. Gowan*. (W. Thacker & Co., London). Although the number of persons who take special interest in Kashgaria may be somewhat limited, as much care has been taken by Col. Kuropatkin with this historical and geographical sketch of the country as if he had a wider audience. The work has been well translated by Major Gowan, and though its subject is rather special it is a valuable treatise.

Dingler's Journal recently described the explosion of an open kettle. At a factory in Neusalz a large cast-iron wash-kettle was used to hold water, into which melted iron was allowed to flow, in a moderate stream, for making iron shot. On October 23, 1882, one of the workmen by mistake allowed the iron to flow too rapidly. There was a sudden development of steam, which threw out a part of the water, frightening the labourer and causing him to drop his ladle, so that about 20 kilogrammes—44 lb.—of melted iron fell at once into the water. There was an immediate rapid outburst of steam and a loud explosion, which shattered the kettle into fragments, tore up the woodwork, threw the workman nearly 8 ft. backwards, and broke his right leg. Only very small pieces of the kettle were found where it stood; some of them were thrown to a distance of about 50 ft.

THE project of a canal across Florida, connecting the Gulf of Mexico with the Atlantic Ocean, is being warmly taken up in America. According to the report of the chief engineer, the total length of the canal will be 139½ English miles. It is proposed to make it wide enough to admit of two steamers passing through it abreast. The cost of the work is estimated at £9,000,000 sterling. When the canal is finished it will diminish the distance between New Orleans and Liverpool or New York by 412 miles. General Stone and his assistants have already completed the preliminary survey. The question the company has to determine is whether the traffic in prospect promises to be sufficiently remunerative to justify the large necessary outlay in realising the project. The chief engineer, in support of his view that the scheme will be a financial success, points out that the shortening of the distance between the Mexican Gulf ports and Europe and the North Atlantic seaboard of America will effect an economy of from three to seven days in time. This means in large vessels a saving of from £60 to £100 a day in food and wages, in addition to a saving of £100 a day in coal. The Straits of Florida are exceedingly dangerous, and shipwrecks there are very frequent. A further saving will accordingly be made in insurance on vessels, which is estimated at from 1 to 1½ per cent. A large increase in the foreign and ocean trade to Texas and the States bordering on the Mississippi will be almost certain to follow the completion of the Canal. The most elevated spot through which the Florida Canal could be cut is considerably lower than the highest point on the route through which the Suez Canal was carried. There are, in fact, no great engineering difficulties to surmount, and the realisation of the scheme is practically only a question of time.—*Engineer.*

"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

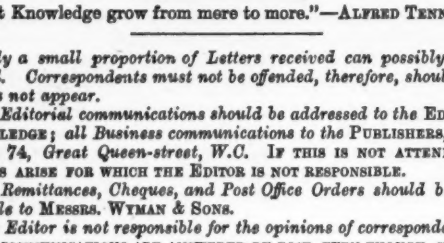
All Remittances, Cheques, and Post Office Orders should be made payable to Messrs. Wyman & Sons.

The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

THE COMET.

[1099]—When looking at the comet a few days ago I observed a separate nebulosity quite distinct from, but near to the tail, and about half way down on the left hand (telescopic). It was circular, somewhat fainter than the tail, and about the same breadth.



Doubtless others will have seen this, but it may be well to draw the attention of any who have not seen it. Senex.

THE AFTERGLOW.

[1100]—I have to thank Mr. Mattieu Williams (letter 1093, p. 4) and Mr. Ekershaw (letter 1095, same page), for their interesting replies to my question as to the condition of the sky where it was clear on Christmas evening.

In reply to Mr. Ekershaw, I have not noticed the glow in actual daylight, but its appearance has occurred very early in the evening, and persisted to an abnormally late hour. Observations of it in actual sunlight are both interesting and important.

With reference to Mr. Ekershaw's idea that the greenness of Venus was a mere retinal effect of contrast, I would refer him to my record of an observation by Mr. Pratt, of Brighton (letter 1040), on p. 365 of your last volume. William Noble.

Forest Lodge, Maresfield, Uckfield,
Jan. 21, 1884.

STRANGE PHENOMENON.

[1101]—Had things been as "A. McD." seems to infer with regard to the "Strange Phenomenon" (1068) which I witnessed in the Persian Gulf, I would certainly not have taken up your space by asking enlightenment on a subject I could easily explain myself.

Though my letter was in no way scientific, still it was none the less true. I don't suppose "A. McD." means any harm, but I do think it's rather unjust to say a man is drunk because he sees something out of the common. If there is one thing that I pride myself in, it is being able to say that never in my life have I indulged in anything stronger than water. What I described in letter 1068 is quite true. I don't pretend to have given the exact dimensions of the wheels, nor the exact time of revolution, but the general appearance was just as I said.

The appearance could not possibly have been caused by lights from cabin windows, as I had seen all lights out at 10 p.m., except, of course, the mast-head, binnacle, and side-lights.

I should like an explanation a little more scientific, but not quite so personal. However, "No offence taken where, I suppose, none is meant." J. W. Robertson.

[We owe our correspondent an apology,—a misunderstood instruction led to the appearance of a letter which, though we are sure well-meant, was not well-advised. The opening criticism, as to the apparent size and velocity was sound enough. But the observation ostensibly recorded only impressions not measurements.—R. P.]

TRICYCLE WHEELS.

[1102]—The advice given by Mr. Browning on the size of tricycle wheels induces me to remark that I think it will be found misleading under ordinary conditions of roads, though in some instances it is correct. I am publishing a treatise especially to inform tricyclists on the various points of construction in a tricycle, and I would ask your readers to understand for themselves what their requirements are before following a personal opinion. So many advantages follow on the use of the "correct" size, that I think when the reasons that should regulate the size of the driving-wheel are known it will be found that Mr. Browning's ideas, though erring on the right side, are rather too "low."

F. Warner Jones.

CRIBBAGE PROBLEM.

[1103]—To find the highest number that can be scored in one deal at six-card cribbage by the dealer, he having power to select all the cards, and to determine the order in which they shall be played. [Solution deferred.] H. H. H.

LETTERS RECEIVED (SUB-EDITORIAL.)

D.M.—Edward Bennis.—An Old Subscriber. Not published. Publishers afraid few readers. Thanks.—Electrician.—Hallyards.—J. D. R.—G. D. E.—R. A. Wilde.—An Admirer of Knowledge on the Whole. "Cakes and Ale," by a figure, oh sad one: not meaning necessarily either one or the other but what Shakespeare figured by them. What is it to you, however, if others like what you dislike, or to those others if you dislike what they like? But to understand that line to recommend the foolish jollity of wine-bibbing days, you must have been scarcely awake.—E. Cuthbert. 1. Sun is moving towards Hercules; rate estimated by M. Otto Struve at about three or four miles per second: probably much greater. Nature of orbit unknown. 2. Other suns moving, on various paths, as yet not connected by science. 3. Doubtless our sun will in time lose his light and heat.—Φύσις. The project seems feasible, but we have not space for its discussion here.—Juan. Clogs and clog-dances outside our knowledge.—More Light. Thanks. Spectrum of phosphorescent bodies not definite like the spectrum of a glowing gas.

Our Mathematical Column.*

ACHILLES AND THE TORTOISE.

By Richard A. Proctor.

Among the problems with which it pleased the ancients to perplex themselves was one which bears in an instructive manner on the doctrine of limits. It may be thus stated:—

The swift-footed Achilles started in pursuit of a tortoise which was 10,000 yards from him, Achilles running 100 times faster than the tortoise. Now, when Achilles had traversed the 10,000 yards, the tortoise had travelled 100 yards; when Achilles had traversed these 100 yards the tortoise had travelled one yard; when Achilles had traversed this yard the tortoise was still one hundreth part of a yard in advance; when Achilles had traversed this hundredth part of a yard the tortoise was the 10,000th part of a yard in advance; and so on *for ever*—the tortoise being at each stage in advance of Achilles by one hundredth part of the distance Achilles had traversed in the preceding stage. The tortoise then remains always in advance of Achilles by some distance however minute; and therefore Achilles can never overtake the tortoise. But we know that Achilles travelling faster than the tortoise *will* overtake it. Therefore Achilles will and will not overtake the tortoise; which is absurd.*

* The ancients were strangely fond of problems of the sort. Thus there was the famous problem about the ass between two exactly equal bundles of hay, at exactly equal distances. "This ass," says the sophist, "will attempt to eat neither bundle; for,

It will be noticed that this problem with its *solvitur ambulando* contains implicitly the very difficulty concerning the method of limits which has caused many to look with doubt on results obtained by this method—imagining that we cannot learn the exact truth by a method which causes us to approach it as near as we please. It is as true on the one hand that we may take any number of the stages considered in the problem without bringing Achilles on a strict level with the tortoise, as that if such a race were actually run the man would at the end of a definite time overtake the animal. And in like manner it is as true that in problems depending on the method of limits we do not obtain exact relations *while* applying the method, as that the result arrived at *by* the method is strictly exact. And that difficulty which in the case of the Achilles and Tortoise Problem some of the ancients chose to regard as insuperable, is the very one that troubles many modern students of mathematics, when they are told of those seemingly contradictory relations which appertain to the theory and method of limits.

I do not know that I can better introduce the doctrine of limits than by taking the above problem as an illustrative case, and showing how the method of limits applied to that problem leads to precisely the same result as the simpler method applicable in this case (and in many others which admit of being solved by the method of limits).

Suppose we take the problem, first, as one to be solved by simple algebraical considerations. We must first assign a definite velocity to Achilles and the tortoise. Suppose that the Swift-footed runs at the rate of a mile in 4 m. 24 s., or 400 yards per minute (our best professional runners covering a mile in 4 m. 20 s., and Achilles having much more than a mile to run). Then the problem would be thus dealt with by the algebraist:—

Let x = time in minutes occupied by Achilles in overtaking the tortoise. Then the space covered by Achilles in yards will be $400\,x$; and the space traversed by the tortoise will be $4\,x$. And since Achilles has in this time gained 10,000 yards on the tortoise we have as our equation for determining x

$$400\,x = 10000 + 4\,x$$

$$\text{or } 396\,x = 10000$$

$$x = \frac{10000}{396} = 25\tfrac{25}{99}$$

in which number of minutes Achilles will overtake the tortoise.

Now the method of limits would be applied somewhat on this wise. Achilles traverses the 10,000 yards in 25 minutes, and is then 100 yards behind the tortoise. He traverses the 100 yards in one 4th part of a minute, and is then 1 yard behind the tortoise. He traverses the yard in one 400th part of a minute, and is then one 100th part of a yard behind. He traverses the one 100th part of a yard in one 40,000th part of a minute, and is then the 10,000th part of a yard behind and so on continually, each stage occupying him one 100th part of the time occupied by the preceding. Hence he will overtake the tortoise in

$$\left[25 + \frac{1}{4} + \frac{1}{400} + \frac{1}{40000} + \frac{1}{4000000} + \&c. + \&c.\right] \text{ minutes} \quad (A).$$

Now setting aside the $25\frac{1}{4}$ minutes for the present, let us see how the other fractions sum up, when we take two, three, four, and so on. We get

$$\frac{1}{400} + \frac{1}{40000} = \frac{101}{40000}$$

$$\frac{1}{400} + \frac{1}{40000} + \frac{1}{4000000} = \frac{10101}{4000000}$$

$$\frac{1}{400} + \frac{1}{40000} + \frac{1}{4000000} + \frac{1}{40000000} = \frac{1010101}{400000000}$$

And it will be noticed that these fractions approach more and more nearly to the value $\frac{1}{396}$. This is easily seen by taking the defect of each from $\frac{1}{396}$. Thus—

$$\frac{1}{396} - \frac{1}{400} = \frac{1}{39600}$$

$$\frac{1}{396} - \frac{101}{40000} = \frac{1}{3960000}$$

$$\frac{1}{396} - \frac{10101}{4000000} = \frac{1}{396000000}$$

$$\frac{1}{396} - \frac{1010101}{400000000} = \frac{1}{39600000000}$$

The difference it will be seen grows continually smaller and smaller, and may be made as small as we please by proceeding far enough. Hence the *infinitely* extended series (A) is equivalent to

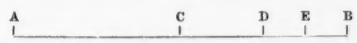
$$\left(25 + \frac{1}{4} + \frac{1}{396}\right) \text{ minutes}$$

that is to $25\frac{2}{3}\frac{5}{3}$ minutes

the same result which we obtained by the direct method.

Now the point to be noticed here is that although the method of limits does not here actually bring us to the exact value *while we are still applying the method*, it shows us the way to that value and *not to an approximate value*.

In order the more clearly to recognise the nature of the approximation which is actually involved in the method of limits we may take a much simpler case. Suppose I have a line of two inches before me as A B in Fig. 1. Then I may suppose this line directly measured and its length to be thus ascertained. But



may also conceive that one-half of it, A C, is cut off, and then the half C D of the remainder, the half D E of what then remains, and so on *ad infinitum*. In this way I should never get to the end B, but I should get as near as I pleased to this point. The point B would obviously be the end to which this process of section would tend; and if I added together the length of all these pieces I should see that the length to which the sum continually approached was as by direct measurement a length of two inches. This *method* of determining the length would be in itself approximate; but the deduced length of two inches would be exact.

Now in both these instances we have gone out of our way (though not without a purpose) to apply the method of limits to matters which can be much better solved by a simpler method. But when the student learns that there are numerous problems—or rather an infinity of problems—which can only be solved by the method of limits, he can see the importance of establishing the *exactness* of the results obtained by the method. The method is *approximative*, but the results obtained by it are exact.

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by whatever line of reasoning it could be shown that he would turn first to one bundle, by a line of reasoning precisely similar it may be shown that he would turn first to the other. But he cannot turn first to both. Therefore, he will turn to neither." Another of these problems was thus worded:—"Epimenides the Cretan says that the Cretans are liars. Now Epimenides is himself a Cretan; therefore Epimenides is a liar. Therefore the Cretans are not liars. Therefore Epimenides is not a liar. Therefore the Cretans are liars. Therefore Epimenides is a liar. Therefore, &c., *ad infinitum*." Others stated the problem in a more simple form, thus:—"When a man says *I lie*, does he lie or does he not lie? If he lies he speaks the truth, if he speaks the truth he lies." We are told that one philosopher, after vainly endeavouring to clear up this important question, flung himself, in despair, into the sea. Philosophy sustained no great loss, it may be conceived.

Our Whist Column.

By "Five of Clubs."

LATE SIGNALS.

IT is well remarked by both Cavendish and Clay that if a player fails to signal at the first opportunity, his partner need not regard a signal given later as having the same authoritative character which an original signal possesses. An original signal means more than a trump lead. It means, or should mean (only some players are too ready to signal), that the signaller is not only very strong in trumps, but has such strength in other suits that (1) he can answer for the absolute safety of a trump lead, and (2) can give good promise of a great game. A signal after the first chance for signalling has passed, means much the same as a trump lead; and, whatever rule to the contrary may be set up, a trump lead does not involve the return of trumps by partner as necessary or even always proper. Very often a trump lead is tentative, and in not a few cases where it is so, the return of trumps would be bad play. So, a late signal means little more, usually, than that a lead of trumps seems likely to be advantageous.

But it occasionally happens that a late signal points to the one sole way of making the game, and should be answered at once. In fact, after the middle of a hand, a signal—if possible, which is not often—acquires a very pointed meaning. Take a case such as occurred to the editor a few evenings ago. We will call the editor *B*, his partner *A*, and players to right and left of *B*, *Y* and *Z*, as in our games. Seven rounds remained to be played, and one round of trumps (diamonds) had been already taken out, in such sort as to leave the best, 3rd, and 5th best trumps with *B*, the 2nd and

4th with *Y* and three trumps between *Z* and *A*, their positions unknown but one certainly with *A*. One trump had been forced from *Y*, the original trump leader. The best and third best hearts lay with *Y* and three small Hearts were with *A*, command in Clubs being with *Z*. *B*, who has not had a lead, holds, besides his three trumps, Queen, Knave, Three, and Two of Spades. As it chanced, every trick was wanted to make the game. At this juncture *A* led Spade King, Spades having been as yet unplayed, but (from the play) being *Z*'s suit. Here *B*'s course to a won game (with *A*'s concurrence) is plain and obvious, while it is equally clear that any other course must lead to the loss of one trick at least by *A*, *B*. The one sole way of making the game is by signalling. Therefore, *B* dropped the Three and Two of Spades in that order to the King and Ace; *A* responded to the signal by a trump lead; and every trick went to *A*, *B*. On the contrary, if *B* had not signalled, or *A* had failed to respond, *A* would have led a small Heart which *Y* would have covered with the third best, and *B* would have been forced to ruff; for if he passed the trick, *Y* would have simply repeated the force. Then *B* could have done nothing with his command in Spades but force the enemy, uncertain whether he were forcing *Z* or *Y*; if *Z*, then the lead of a winning Club would again force *B*, and three tricks in all would be made by *Y*, *Z*; if luckily *Y*, then but one trick would be made by *Y Z*, but still their game would be saved.

Some players seem to think that if they look out for the signal in the first suit led they have done all that is required of them; but the above case and others which might easily be cited show that even towards the end of a hand the signal may be played with effect; and that therefore it should be looked for to the last. In other words, it is always worth while to attend, to the very last, to the play of the small cards.

PROBLEMS.

The two following problems were contributed several years ago by Mr. Lewis to the *Westminster Papers*:—

Simple Ending No. 1.

Y's cards. *C K*; H Kn; S Kn, 2; D 9.
A's cards. H A, 10; S 10; D Kn, 7.
B's cards. *C A*, 5; S 4; D 4, 2.
Z's cards. H K, 5; S 6; D 10, 5.

Clubs trumps; *A* to lead; *A B* to make all five tricks.

Simple Ending No. 2.

Y's cards. *C* 10, 7; H A, Kn; S Q, 10, 2.
A's cards. *C A*, Kn; H 6; S 4; D 10, 9, 8.
B's cards. H K, 7, 4; S K, 9; D A, Q.
Z's cards. *C* 9, 6; H 9, 8; S 8, 7, 6.

Clubs trumps; *A* to lead; *A B* to make all seven tricks.

Our Chess Column.

By MEPHISTO.

SELECTED PROBLEMS.

By W. A. SHINKMAN.

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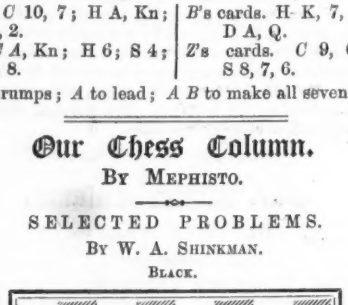

WHITE.

White to play and self-mate in four moves.

By E. N. FRANKENSTEIN.

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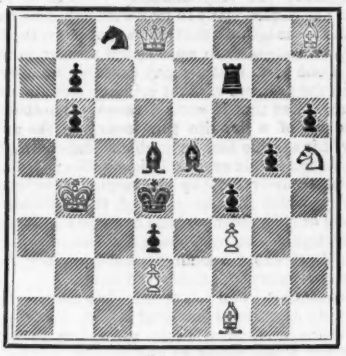

WHITE.

White to play and mate in four moves.

By the Rev. H. BOLTON.

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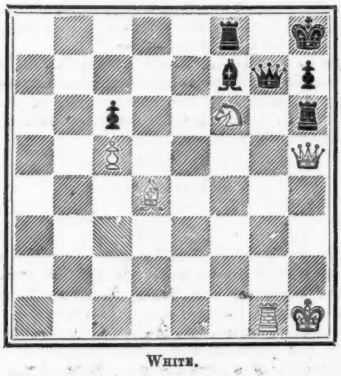

WHITE.

White to play and mate in seven moves.

THE Index to Vol. IV., KNOWLEDGE, is now ready, price 2d.; post-free, 2½d. The Volume also is just published comprising numbers from July to December, 1883, price 7s. 6d. Office: 74 to 76, Great Queen-street, London, W.C.

SPECIAL NOTICES.

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1. LIFE OF WORLDS.
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3. THE MOON.
4. THE PLANETS.
5. COMETS.
6. THE STAR DEPTHS.

See Advt. Pages for full Syllabus.

The following arrangements are complete: the numbers in brackets referring to above list.

BRISTOL (Colston Hall), Feb. 19, 22, 26, 29; March 4, 7 (the full course).
CHELTENHAM (Assembly Rooms), Feb. 5, 8, 12, 15 (1, 2, 6). At 3 o'clock, Feb. 5 and 12 (3, 5).
BATH (Assembly Rooms). Four Morning Lectures at 3 o'clock, Feb. 6, 9, 13, 16 (1, 3, 4, 6); two Evening Feb. 6, 13 (2, 5).
BIRKENHEAD, March 10.
ALTRINCHAM, March 11 (5).
CHESTER, March 12, 13 (1, 2).

NOTE.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*